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Miscellanea nomenclatorica batrachologica. 19. Notes on the nomenclature of Ranidae and related groups

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The need of "working taxonomies", as tools providing a framework for alpha-taxonomic revisionary works and hypotheses for phylogenetic analyses, is pointed out, especially in groups with wide distribution and high number of species. Even during the transitional period, non-ambiguous communication between zoologists requires that use of names for taxa strictly follows the international rules of zoological nomenclature. Several cases of recent nomenclatural problems are pointed out in the "family Ranidae". Rediscovery of the generic name *Chilixalus* Werner, 1899 provides an opportunity for discussing several careless treatments of the generic and infrageneric taxonomy of frogs of the "genus *Rana*" by recent authors. The name Indiraninae Blommers-Schlosser, 1993 is shown to be an invalid junior synonym of Ranixalinae Dubois, 1987, and, on this occasion, the fact that family-group names are regulated by the rule of priority is reminded once again. In order to help knowing the valid name among several names published simultaneously, in the case they are considered subjective synonyms, two tables of first-reviser actions in the Ranidae and related groups are presented. Finally, discussion of the nomenclatural status of the name "*Rana duboisi*" recently published by EMERSON & WARD (1998) allows to point to the problems posed by the publication of data taken from unpublished manuscripts by colleagues, either submitted to review by an editor, or privately communicated by the author or another person. This case is also the basis for the discussion of two more general questions, which are likely to appear again on several occasions in the years to come: is a species name rendered nomenclaturally available by publication, either (1) of a Genbank catalogue number, or (2) of a cladogram including this species? The answer to both questions is clearly "no", at least under the current *Code*. Allocation of names to taxa is not based on definitions, diagnoses or descriptions, but on the taxonomic allocation of name-bearing type-specimens: the recent proposal of attaching the names to "phylogenetic definitions of taxon names" is therefore based on a major misunderstanding and entertains an unnecessary confusion between taxonomy and nomenclature, as the current nomenclatural system is liable to accomodate any kind of taxonomy, including "phylogenetic" ones.

With more than 750 species (GLAW et al., 1998) distributed almost worldwide, the "family Ranidae" is one of the largest amphibian higher taxa. Its taxonomy is still very problematic, if not really "in a state of chaos" (DUELLMAN & TRUEB, 1985: 544). Resolution of all the problems it raises will be a long task, as it will require a high number of works of

various kinds (morpho-anatomical, molecular, cytogenetic, bioacoustic, ethological, ecological), both at local scale and at world scale, and dealing both with alpha-taxonomy and with higher classification. Only when this is done can we hope to have a reasonably good knowledge of the species of the family and understanding of their phylogenetic relationships. Given the high number of species concerned, and the extremely large distribution of the group, it is impossible for any researcher to work on the whole of the family at once. Before applying any technique or carrying out any survey, choices must be made among the hundreds of species of the family. This choice can be made along three major lines: (1) on a geographical basis, i.e. studying the ranids of a given region of the world; despite the fact that this approach is clearly unsatisfactory (see e.g. DUBOIS, 1981, 1987*a*, 1992), for material reasons this has been the case of the vast majority of works dedicated to the taxonomy of this family until now, (2) according to the availability of specimens in one or several museum or other collection(s), which may be a little better when rich, largely representative collections are used, but remains unsatisfactory in most cases; (3) on a provisional taxonomic basis, which is clearly the best starting point for any revisionary taxonomic work (see e.g. MAYR, 1981). Some zoologists (e.g., INGER, 1996) seem to think that a taxonomy is only a *result*, and that taxonomies should only be established, or changed, when biologists have "final" data on the (cladistic or other) relationships between the species. This is a very reductory and misleading view of the rôle of taxonomy in biology, for two reasons at least:

(1) Most data on which taxonomies are based are conventional (i.e., based on subjective choices, e.g. as to which kind of information should be provided by the classification) and/or hypothetical (e.g., cladograms as hypotheses of cladistic relationships). As a consequence, no taxonomy is or will ever be the "final" one, for any group of living beings. Asking to postpone the establishment or change of taxonomies until we have "final" data is just a way to say that no taxonomy can ever be established, or that the existing taxonomies (often inherited from "tradition", both in its best and worse senses), should never be changed, for reasons of "stability of nomenclature" However, no nomenclature can ever be completely stable, if taxonomy is to remain a living, i.e. evolutive, science (see DUBOIS, 1998).

(2) Such requests ignore one of the functions of taxonomies, i.e. their "heuristic value": a taxonomy is not only a result, it is also a *starting point*. It is a hypothesis, that can be tested by further research and modified. This is particularly true in speciose and complex groups with large distributions, that cannot be comprehensively studied at once, such as the Ranidae: in these groups, at least if one really wishes to improve the existing taxonomies, provisional groupings as "phenetic taxa" (such as e.g. the "phenetic groups" recognized in toads of the genus *Bufo* by some authors: DUELLMAN & SCHULTE, 1992, DUBOIS & OHLER, 1999) are necessary to have a "working taxonomy". Such groups of a reasonable size, chosen neither on a geographical nor on "availability" grounds, will allow real, although partial, revisionary works. This is demonstrated by the fact that most of those who recently really tried to improve the taxonomy of ranids above the species level (and not only to "comment" on the work of others), had to start from subsets of the whole family, which, although they might not have stated it, were those proposed as "working taxa" by previous authors (see e.g. EMERSON & BERRIGAN, 1993; EMERSON, 1996; EMERSON & WARD, 1998)

The request for stability of taxonomies and nomenclatures, that some authors (e.g. INGER, 1996) wish to apply to the Ranidae[1], is relevant only for some zoological groups, either of very small size (e.g., higher primates) or already very much studied (e.g., birds), for which an enormous wealth of information is already available, and in which competing taxonomic schemes only or mostly depend on subjective choices as to the major criteria to be taken into account in the building of classifications (classification or "cladification" MAYR, 1997; DUBOIS, 1997), on different weightings of the characters, etc. But in poorly known groups like the Ranidae, we strongly need provisional, working taxonomies, to really help progress of research and to guide future alpha-taxonomic works and phylogenetic studies. Such temporary taxonomies can be progressively modified and replaced by better ones, as information becomes available, but just to obtain this information may be very time-consuming. Pending its obtention, zoologists cannot be left in a "non-taxonomic land". They need "working taxonomies" and "working nomenclatures". In such groups, a fascination, or a quasi-religious respect, for "stability" of taxonomy and nomenclature, can work as a break against increase and improvement of our knowledge. Of course, in such enormous groups as the Ranidae, where no researcher in the world can have access to all, or even to a high proportion of, the described species, and where many species are known only by a very low number of specimens, sometimes only in the adult stage (the tadpoles being unknown), such a provisional taxonomy can only be based on a heterogeneous combination of various sources of information: some based on field work, some on detailed anatomical studies, some on examination of specimens limited to external characters, and some on data published by previous authors. Because of this unavoidable diversity of sources of information, the data set is bound to be incomplete, as some character states (particularly those requiring dissection and anatomical study, or those of tadpoles) will be known for some taxa only: such data cannot therefore be used to build up a matrix and carry out a phylogenetic analysis, but can allow partial, provisional definitions of phenetic groups, diagnosed by characters shared only by their included species and that can in a first step be hypothesized to be synapomorphies of the latter. Of course, such a work is likely to include some mistakes, but then a useful contribution of subsequent workers will be to correct these and improve the provisional

1 Actually, the motivation for writing this paper (INGER, 1996) are difficult to understand. Although this author has published numerous papers on the Oriental, Asiatic and African frogs for more than half a century, he has never shown real interest in the phylogeny and supraspecific taxonomy of these groups, as he never provided a significant contribution to this field but merely perpetuated BOULENGER's ideas and schemes in this respect. Some of the information provided in his recent paper could have been proposed as a constructive contribution to the taxonomy of ranids, and will no doubt be used as such in the future. This is indeed the kind of information I was expecting when I wrote my "proposals" (DUBOIS, 1992), which are clearly a basis for discussion and improvements, not a "final system" although my paper was the result of research over a 20-year period, it is clear that I could not have examined all ranid groups worldwide. However, instead of proposing these comments as positive elements for correcting and improving my proposals, INGER's (1996) paper is only negative and aggressive, and does not offer alternate proposals but "waiting for more data", to paraphrase KOTTELAT's (1997: 2, 4) nice words (see also DUBOIS & OHLER, 1999: 135). This casts some doubts on the real intentions of its author (and of the editors of the journal where it was published). The aim of this publication was clearly not to open a debate (which could well have been published in the "points of view" of this journal); this is why I did not think it worthwhile to write a reply. Replies will come slowly but surely as the much needed works on the ranids are progressing (see e.g. MARMAYOU et al., in press). In the meantime, following INGER's "traditional" taxonomy is not likely to help authors who are really interested in the relationships within the Ranidae, and who need subgroups to deal with the hundreds of species still allocated by some to the genus *Rana*: presenting these species in a publication by alphabetical order of specific names is highly misleading, as is the use of arbitrary groups based on grossly incomplete data, as well exemplified by the recent "sections" recognized by TIAN et al. (1995) in this genus, which ignore many important pieces of information published after BOULENGER's works.

taxonomy, rather than reject it altogether without using its good parts. This is the way science usually progresses, particularly in biology (see e.g. MAYR, 1982, 1997).

Taxa, even provisional, must be named (DUBOIS, 1988), and the fact that they are provisional does not mean that their nomenclature should not be rigorous. Strictly following the rules of the *International Code of Zoological Nomenclature* (ANONYMOUS, 1985; quoted below as "the *Code*") is a guarantee of *non-ambiguous, stable, automatic and universal allocation of names to taxa*, which is much more important than the (highly praised by some) "stability of taxa and names". Among these rules, the rule of priority is an important one. A recent tendency has developed among some zoologists (e.g., SAVAGE, 1990*a-b*, 1991; BOCK, 1994) and in decisions of the International Commission on Zoological Nomenclature (quoted below as "the Commission") to severely limit its application in zoology. In so doing, the Commission has not properly played its rôle of "Keeper of the Law" (DUBOIS & OHLER, 1997: 299), and has encouraged neglect of all the rules by zoologists. No doubt such attitudes contribute to the current weakening of the binding legislative status of the *Code* for the establishment of the valid names of taxa in the eyes of many taxonomists. Such a movement may have important negative consequences in the long run regarding the existence of a unique international nomenclatural system, and therefore the unity and universality of zoology as a science (DUBOIS, in preparation).

In the recent years, and largely as a result of this tendency, a number of nomenclatural problems in amphibians have been pointed out (see e.g. DUBOIS, 1987*a-b*, 1995, 1998, DUBOIS & OHLER, 1995, 1998). The purpose of this paper is to present a few new such problems in the Ranidae and their solutions, within the frame of the current taxonomy of this family. This does not preclude the possibility that the nomenclature of the taxa discussed below may have to be changed in the future, as the taxonomy of these groups evolves, nomenclature being at the service of taxonomy (and not the reverse), names will necessarily have to change as taxa are modified, suppressed or created. The particular cases presented below will also provide the opportunity to discuss several more general problems of zoological nomenclature and of scientific publications.

CHILIXALUS WERNER, 1899

SCHMIDT (1857: 11) published a preliminary diagnosis of *Ixalus warszewitschii*. Shortly after, the same author (SCHMIDT, 1858: 241-242, 258, pl. 1) provided a more detailed description of this species, for which he gave a precise type-locality, now situated in Panama (see HILLIS & DE SÁ, 1988: 15). For the name of the species, he then used two spellings: on page 258, the correct original spelling *Ixalus warszewitschii*, and on page 241, on two occasions, the spelling *Ixalus warschewitschii*, which must therefore be viewed as an incorrect subsequent spelling, as noted by HILLIS & DE SÁ (1988: 16). Most subsequent authors (e.g., DUNN, 1931: 416, TAYLOR, 1952: 896; GORHAM, 1974: 153, FROST, 1985: 520) used this latter spelling, until HILLIS & DE SÁ (1988: 1) resurrected the correct original spelling.

SCHMIDT (1858: 242) stated that FITZINGER had privately suggested to him that this species should be placed in a new genus, but he refrained from doing so until field work could

bring more data about it. However, another, subsequent author did not have as many scruples and created a nominal genus for this species without having additional information: WERNER (1899: 117) proposed the new generic name *Chilixalus* for the nominal species "*Ixalus warszewiczii* Schmidt". He had apparently not seen the publications of SCHMIDT (1857, 1858) on this species, as he stated that he had found this name on a bottle in the Krakow Museum containing a frog specimen from "Neu Granada". Fortunately, he mentioned the collection number (1006) of this specimen, which is the same as that reported by HILLIS & DE SÁ (1988: 15) for SCHMIDT's (1857) holotype, so that there is no doubt about the fact that WERNER (1899) actually dealt with the same species as SCHMIDT (1857, 1858). WERNER's (1899) spelling of the specific name, repeated twice in his paper, was clearly intentional, and should be regarded either as an unjustified emendation of SCHMIDT's original name, or, because of WERNER's ignorance of SCHMIDT's publications, as the name of a new nominal species: in both cases, the name *Chilixalus warszewiczii* has an independent status in nomenclature and is a junior objective synonym of *Ixalus warszewitschii* Schmidt, 1857

Although published by a well-known zoologist in a major journal, the name *Chilixalus* has been almost completely forgotten by subsequent authors. It was mentioned by BOULENGER (1900*b*: 28; 1910: 152) and NEAVE (1939: 691), but ignored altogether in all major works dealing with the classification of Ranidae and Rhacophoridae, or with the ranids of Central America: e.g. GÜNTHER (1900), BOULENGER (1920), AHL (1931), NOBLE (1931), TAYLOR (1952), GORHAM (1974), DUBOIS (1981, 1992), DUELLMAN & TRUEB (1985), FROST (1985) and HILLIS & DE SA (1988). Although close by its spelling to the generic names *Chirixalus* Boulenger, 1893 (Ranidae, Rhacophorinae) and *Callixalus* Laurent, 1950 (Hyperoliidae, Hyperoliinae), the name *Chilixalus* differs from these names by one or two letters, and is therefore not their homonym. The type-species of this nominal genus is now considered a member of the genus *Rana* Linnaeus, 1758, as *Rana warszewitschii* (Schmidt, 1857) (HILLIS & DE SÁ, 1988)

Fortunately, rediscovery of the name *Chilixalus* does not have disturbing effects on nomenclatural stability, whatever the classification scheme chosen. Three classification schemes are currently applied by different authors to the group of ranids including the nominal species *Ixalus warszewitschii* Schmidt, 1857: (1) for authors who do not recognize subgenera in *Rana*, it is a member of the *Rana palmipes* group; (2) for some authors, it is a member of the subgenus *Lithobates* Fitzinger, 1843 of *Rana*; (3) according to the provisional classification of ranids proposed by DUBOIS (1992), it is a member of the subgenus *Typheropsis* Cope, 1868 of *Rana*

Unfortunately, a certain amount of taxonomic and nomenclatural vagueness applies to several recent works dealing with the "*Lithobates* section" (DUBOIS, 1992: 323, 329) of the genus *Rana*. Although the title and abstract of their paper only referred to the "*Rana palmipes* group", HILLIS & DE SA (1988: 16-17) suddenly mentioned a "subgenus *Lithobates*", without stating its author, date and content, and without reference to a publication where this would appear. In support of this use, they gave two references, one to a paper (HILLIS & DAVIS, 1986) where the "subgenus *Lithobates*" was not at all mentioned, and one to an unpublished thesis (HILLIS, 1985), where a subgenus "*Lithobates* Fitzinger" (without date) was briefly mentioned (p. 266-267), without any reference allowing to identify this name. As a matter of fact, as of 1988, the status of the name *Lithobates* had been discussed in only one publication (DUBOIS,

1981: 249-250), not quoted by these authors, where this name was considered a synonym of *Rana*. Therefore, HILLIS & DE SÁ (1988) resurrected the generic name *Lithobates* for a new subgenus for which they did not provide a diagnosis, and without discussing, even briefly, the status of the other subgenera recognized until then in *Rana* (DUBOIS, 1981, 1987*a*).

The taxonomy of the American species, species-groups and subgenera currently referred to the genus *Rana* will not be definitively clarified until their relationships with non-American (i.e., European and East Asian) species of this "genus" are studied in detail, and any current taxonomic scheme can only be considered as a provisional, working taxonomy. I provide below synonymies of the subgenera provisionally recognized by DUBOIS (1992: 329-331) in his "*Lithobates* section" of the genus *Rana*: these are phenetically diagnosable groups for which a hypothesis of cladistic relationships has been proposed by HILLIS & DE SÁ (1988: 18). Under this scheme, the generic name *Chilixalus* appears as a junior subjective synonym of *Trypheropsis*. Under the other classification schemes mentioned above, it is either a junior subjective synonym of *Lithobates* or a junior subjective synonym of *Rana*.

Lithobates Fitzinger, 1843

Lithobates Fitzinger, 1843: 31 – Type-species by original designation: *Rana palmipes* Spix, 1824: 29.
Ranula Peters, 1859: 402 (nec Schumacher, 1817: 77) – Type species by monotypy: *Ranula gollmeri* Peters, 1859: 402.
Pohlia Steindachner, 1867: 15 – Type-species by monotypy: *Rana palmipes* Spix, 1824: 29.

Sierrana Dubois, 1992

Sierrana Dubois, 1992: 330 – Type-species by original designation: *Rana sierramadrensis* Taylor, 1939: 385.

Trypheropsis Cope, 1868

Trypheropsis Cope, 1868: 117 – Type-species by original designation: *Ranula chrysoprasina* Cope, 1866: 129.
Levirana Cope, 1894: 197 – Type-species by monotypy: *Levirana viricaria* Cope, 1894: 197.
Chilixalus Werner, 1899: 117 – Type-species by monotypy: *Chilixalus warszewiczii* Werner, 1899.
Laevirana Gunther, 1900: 206 – Unjustified emendation of *Levirana* Cope, 1894.

Zweifelia Dubois, 1992

Zweifelia Dubois, 1992: 330 – Type-species by original designation: *Rana tarahumarae* Boulenger, 1917*b*: 416.

OTHER INCONSISTENCIES IN RECENT TREATMENTS OF RANID TAXONOMY AT GENUS AND SUBGENUS LEVEL

The case of the "subgenus *Lithobates*" just discussed is not unique in the recent literature dealing with ranid taxonomy. To tell the truth, it is difficult in many cases to be sure of what infrageneric taxonomy is indeed followed by many current authors within the genus *Rana*, which clearly points to a general uneasy feeling in this respect. Thus, in a paper dealing with some Asian frogs, EMERSON & BERRIGAN (1993) mentioned a "subgenus *Limnonectes* (Fitzinger)" in their title, but did not clearly state in the text which species they included in this subgenus, nor in other "subgenera" of their very comprehensive "genus *Rana*". They introduced (p. 23) the new combination "*Rana* (*Occidozyga*) *cyanophlyctis*" without any comment, which seems to imply that they recognized a new subgenus *Occidozyga* in the genus *Rana*. However, they did not propose a diagnosis or definition of the latter, nor did they define its content. Did they mean that all species shown in the consensus tree of their figure 8 should be included in this subgenus, or should the latter be understood as comprising only some species of this tree, namely *lima* (type-species of *Occidozyga*: see DUBOIS, 1981), *limnocharis*, *cancrivora* and *cyanophlyctis*? This information is not to be found in their paper. It is surprising to see publication of such non-professional treatments of taxonomic and nomenclatural matters in a well-known herpetological journal, but this is only one example of a recent tendency for zoological publications, even of high level, to ignore the basic taxonomic and nomenclatural rules.

In a later paper, EMERSON (1996: 279) first expressed high concern for the proper use of scientific names in biological publications. "until a proper systematic treatment is completed, it seems premature and potentially confusing to use the name *Limnonectes* in the literature. In this paper, members of that group will be referred to as the fanged frogs and their relatives." However, a few pages below in the same article, she seemed to have forgotten these good resolutions, as she presented quite confusing information. In page 281, she wrote. "Egg size was measured in adult females of 19 species of fanged frogs and 16 species of outgroup ranids belonging to the genus *Hylarana*". In the legend of her figure 2 (p. 282), "outgroup species of the genus *Hylarana*" appear again, but the text of the same page mentions "outgroup ranids belonging to the subgenus *Hylarana*". Genus or subgenus? Actually, until now, while most zoologists working in Africa consider *Hylarana* Tschudi, 1838 as a genus, no author working on Asian frogs has treated *Hylarana* as a full genus, except FEI et al. (1991) and YE et al. (1993), in two works not cited in the *References* of EMERSON's (1996) paper, so that treating Oriental *Hylarana* as a full genus would seem to have required at least a short comment. Furthermore, DUBOIS (1987*a*: 42) pointed out that Asian species of "*Hylarana*" did in fact represent several clearly distinct groups, and later (DUBOIS, 1992) distributed these species in several sections and subgenera of the genus *Rana*. It would therefore be important to know which are the "16 species of outgroup ranids belonging to the genus *Hylarana*" studied. Unfortunately, EMERSON's (1996) paper does not contain a list of the species, not to say of the specimens, examined. Contrary to her initial statement, EMERSON's (1996) taxonomic treatment of the Ranidae is very difficult to understand and highly confusing. On one hand, she recognized a genus (or subgenus?) *Hylarana* distinct of *Rana* for a heterogeneous group of frogs that by all skeletal and morphological characters have long been known to be rather closely related to the group including the type-species of *Rana* (*Rana temporaria* Linnaeus, 1758). But on the other hand,

allegedly to avoid "potential confusions" linked to the use of the name *Limnonectes*, she maintained in *Rana*, without comment and without mention of subgenera, several groups of ranids long known to be only distantly related to the latter group, including some that have been for more than 150 years (since TSCHUDI, 1838) placed in one or two genera (*Occidozyga* Kuhl & Van Hasselt, 1822 and sometimes *Phrynoglossus* Peters, 1867) distinct from *Rana*. All these taxonomic and nomenclatural novelties were presented in this paper without explanation or discussion, and published in a famous journal whose title claims interest in "systematic biology".

INDIRANINAE BLOMMERS-SCHLÖSSER, 1993 AND NYCTIBATRACHINAE BLOMMERS-SCHLÖSSER, 1993

I have on several occasions already (e.g., DUBOIS, 1984: 6, 1987*b*: 117-126, 1987*c*: 48-52) pointed out that, contrary to what some zoologists seem to believe, family-group names in zoology are regulated by the *Code* and must follow the rule of priority, just like species-group and genus-group names. This means in particular that the valid name of a family-group taxon is the first published one based on a generic name included in the taxon, whatever the current status of this generic name (valid name or invalid junior synonym), this is exactly parallel to the situation in the genus-group, where a genus name can be valid even if its type-species is a junior synonym. This rule is by far the best one for the stability of names, for reasons that were explained in detail already by MYERS & LEVITON (1962), and that may be well illustrated by a simple example (see DUBOIS, 1984). GÜNTHER (1858) established a family Polypedatidae, based on the generic name *Polypedates* Tschudi, 1838. This family was recognized under this name by many authors for many years, including in the title of a volume of the famous series *Das Tierreich* (AHL, 1931), but its name was changed into Rhacophoridae by HOFFMAN (1932) because the genus *Polypedates* was then considered a synonym of *Rhacophorus* Kuhl & Van Hasselt, 1822. Since then however, a tendency has developed to revalidate *Polypedates* as a valid genus : it would clearly have been better, for the sake of nomenclatural stability, to keep using the name Polypedatidae for the family, whatever the status of the generic names. A quite similar case concerns the use of Microhylidae Gunther, 1858 instead of Gastrophrynidae Fitzinger, 1843 (see DUBOIS, 1984). In both these cases, by virtue of Article 40 of the *Code*, the senior names (Polypedatidae and Gastrophrynidae) cannot be resurrected now, as they were replaced because of a synonymy of the type-genus before 1961, but it is important to note that the same would not be true if this replacement had taken place after 1960.

A number of recent authors do not understand this rule, or deliberately refuse to follow it, and they tend to change the name of a family group taxon as soon as its type-genus is considered an invalid junior synonym, even when this was first done much after 1960; they may even clearly argue in favour of this non-respect of the *Code* (see e.g. SAVAGE, 1986). Strikingly, in several recent cases, their attitude was supported by the Commission itself, in accepting to "suppress" some senior synonyms in the family group in order for the corresponding taxa to bear names based on valid generic names. If followed by all, this movement would lead to suppress the rule of priority for family-group names, thus going back to a situation where local groups of zoologists, the most "powerful" ones probably, could try and

impose "their" nomenclature to the whole international scientific community – a most retrograde step indeed.

Interested readers can find a list of such problems in living amphibian family-group nomenclature in DUBOIS (1984, 1987*a*: 11-12, 1987*b*: 121-122, 1987*c*: 48-52). Particularly striking are the cases of two invalid names that are still used by some authors (and accepted by the editors of some journals, even well-known ones), despite these repeated mentions of their invalidity, and although in these cases Article 40 does not apply and does not permit to keep them, i.e. the invalid Xenopodinae Fitzinger, 1843 instead of the valid Dactylethrinae Hogg, 1838, and the invalid Bombininae Fejérváry, 1921 instead of the valid Bombinatorinae Gray, 1825.

I here wish to point out a new case of the same kind. DUBOIS (1987*a*: 66) established a tribe Ranixalini for the ranid genera *Nannophrys* Gunther, 1869, *Nyctibatrachus* Boulenger, 1882 and *Ranixalus* Dubois, 1986. Some months later, however, the same author (DUBOIS, 1987*d*) discovered that the generic name *Indirana* Laurent, 1986 was a senior subjective synonym of *Ranixalus* and should replace it; however, he remarked that, according to the *Code*, the name Ranixalini did not have to be changed and remained the valid one for the tribe. DUBOIS (1992: 334) raised this tribe to the rank of subfamily, under the name Ranixalinae. BLOMMERS-SCHLÖSSER (1993) presented a new hypothesis of cladistic relationships within the Ranidae and proposed two new subfamilies, the Indiraninae and the Nyctibatrachinae. The first of these names is clearly an invalid one, being a strict junior subjective synonym of Ranixalinae: if subsequent authors wished to adopt BLOMMERS-SCHLOSSER's (1993) taxonomic scheme, they should replace the name Indiraninae by Ranixalinae in this classification. As for the name Nyctibatrachinae, it is also likely to be a synonym of Ranixalinae, but here for taxonomic, not nomenclatural, reasons. BLOMMERS-SCHLÖSSER (1993) did not take into account in her analysis the characters pointed out by DUBOIS (1987*a*, 1992) and that suggest that *Indirana*, *Nannophrys* and *Nyctibatrachus* most likely constitute a holophyletic group, such as the presence of femoral glands in males of *Indirana* and *Nyctibatrachus*, the highly derived terrestrial tadpoles showing several probable synapomorphies in *Indirana* and *Nannophrys*, and the characters shared by *Nannophrys* and *Nyctibatrachus* according to CLARKE (1983: 395). Despite these data, BLOMMERS-SCHLOSSER (1993) referred these three South Indian and Sri Lankan genera to three distinct subfamilies (she included *Nannophrys* in her African subfamily Cacosterninae). Therefore, the whole phylogenetic, taxonomic and nomenclatural scheme proposed by this author seems highly questionable and will have to be re-evaluated.

RELATIVE PRIORITY BETWEEN SIMULTANEOUS NAMES

As shown by these examples, it is clear that the taxonomy of the Ranidae is still not stabilized and will show important changes in the future. As argued above, the existence of provisional taxonomic schemes, if well understood, can provide a strong help for the progress of our works on this taxonomy. Taxonomic changes will in their turn result in nomenclatural changes, but, if the proper care is taken, these latter changes will be automatic and should not pose any problem to taxonomists. The previously published catalogues of genus-group and

Table 1 – Past first-reviser actions concerning simultaneous (i.e., published at the same date) genus-group and family-group names in the families Hyperoliidae, Phrynobatrachidae and Ranidae (as defined by DUBOIS, 1992). The sign > means "afforded priority over"

Original publication	First-reviser	Relative priority afforded to simultaneous names by first-reviser action
TSCHUDI, 1838	DUMÉRIL & BIBRON, 1841: 515	*Polypedates* > *Boophis* + *Buergeria*
TSCHUDI, 1838	STEJNEGER, 1907: 143	*Polypedates* > *Buergeria* + *Theloderma*
FITZINGER, 1843	FITZINGER, 1843: 31	*Pelophylax* > *Euphlyctis* + *Limnophilus* + *Phrynoderma*
FITZINGER, 1843	DUBOIS, 1976: 1112	*Euphlyctis* > *Limnonectes* + *Phrynoderma*
BOULENGER, 1882	DUBOIS, 1987*a*: 68	*Nyctibatrachus* > *Nannobatrachus*
NOBLE, 1931	DUBOIS, 1982: 135-136	Petropedetinae > Cacosterninae
LAURENT, 1944	LAURENT & COMBAZ, 1950: 277	*Afrixalus* > *Acanthixalus* + *Heterixalus*

family-group names available in the Ranidae (DUBOIS, 1981, 1984, 1987*a*, 1992) should allow any author to find if names are already available for any newly defined taxon, which one has priority, or if a new name has to be coined. The only cases when nomenclatural changes will not be automatic are those where different names, initially published in the same work or in different works but at the same date ("simultaneous names"), are considered subjective synonyms. In such cases, according to the *Code*, relative priority among these names is fixed by a first-reviser action. Once published, a first-reviser action is definitive and cannot be modified by subsequent authors. It is therefore important to be able to trace all first-reviser actions ever taken in the nomenclature of a given zoological group, but it is a difficult work for anyone who is not very well acquainted with all the literature dealing with the taxonomy of the group.

In order to help future workers on the taxonomy of Ranidae and related groups (recognized as the families Arthroleptidae, Astylosternidae, Hemisotidae, Hyperoliidae, Mantellidae and Phrynobatrachidae by DUBOIS, 1992: 309), I provide in tab. 1-2 a list of publications where several simultaneous genus-group and family-group names currently referred to these groups were published, with information on first-reviser actions and on the resulting relative priority among these names. Only names created in the same publication were surveyed. Problems of priority may also occur between names published in different works of the same year, but then a careful study of the case, with research of information on exact dates of publication, must be carried out, which was beyond the scope of the present work. Table 1 gives information on first-reviser actions that have already been taken by previous authors in the past. When no such first-reviser action has already been published, I hereby take such an action (tab. 2), so that in the future any allocation of genus-group or family-group name in these families should be automatic (except in the rare possible cases of names published in different works of the same year, as mentioned above) and not liable to be complicated by subsequent "inadvertent" first-reviser actions in obscure publications (e.g., due to subjective synonymisation of two names, one being cited in the synonymy of the other) that may escape the attention of some colleagues. The choice of the order of priority among

Table 2 New first-reviser actions concerning simultaneous (i.e., published at the same date) genus-group and family-group names in the families Arthroleptidae, Astylosternidae, Hemisotidae, Hyperoliidae, Mantellidae, Phrynobatrachidae and Ranidae (as defined by DUBOIS, 1992). The sign > means "afforded priority over". Names followed by an asterisk were also concerned by past first-reviser actions (see tab. 1), in such cases, the new first-reviser actions below are compatible with these earlier actions.

Original publication	Relative priority afforded to simultaneous names by present first-reviser action
KUHL & VAN HASSELT, 1822	*Rhacophorus* > *Occidozyga*
TSCHUDI, 1838	*Hylarana* > *Polypedates** > *Boophis** > *Pyxicephalus* > *Buergeria** > *Strongylopus* > *Theloderma** > *Cornufer* > *Oxyglossus* > *Eucnemis* > *Orchestes* > *Oxydozyga*
DUMÉRIL & BIBRON, 1841	*Tomopterna* > *Limnodytes* > *Ixalus*
FITZINGER, 1843	*Pelophylax** > *Euphlyctis** > *Limnonectes** > *Lithobates* > *Hydrophylax* > *Tachycnemis* > *Trachyhyas* > *Eremiophilus* > *Limnophilus** > *Phrynoderma**
GISTEL, 1848	*Philautus* > *Zoodioctes* > *Buccinator* > *Dendricus* > *Phyllodytes* > *Epipole*
SMITH, 1849	*Arthroleptis* > *Stenorhynchus*
GÜNTHER, 1859	*Platymantis* > *Leptopelis* > *Hemisus* > *Sphaerotheca*
PETERS, 1863	*Hoplobatrachus* > *Hemimantis* > *Leptoparius*
COPE, 1865	*Amolops* > *Staurois*
PETERS, 1867	*Phrynoglossus* > *Leptomantis*
GÜNTHER, 1869	*Nannophrys* > *Megalixalus*
HOFFMANN, 1878	*Fergusonia* > *Aemolops* > *Cruminifera*
BOULENGER, 1882	*Mantella* > *Nyctibatrachus** > *Nannobatrachus**
SCHULZE, 1890	*Crotaphitis* > *Baliopygus*
BOULENGER, 1893	*Chirixalus* > *Phrynoderma*
PALACKY, 1898	*Rhacoforus* > *Nannofrys*
BOULENGER, 1900*a*	*Trichobatrachus* > *Cardioglossa* > *Scotobleps* > *Gampsosteonyx* > *Dilobates*
BOULENGER, 1917*a*	*Ptychadena* > *Aubria*
METHUEN, 1920	*Gephyromantis* > *Trachymantis*
AHL, 1924	*Pseudocassina* > *Tornierella*
AHL, 1925	*Hylarthroleptis* > *Pararthroleptis*
HEWITT 1926	*Arthroleptella* > *Microbatrachella* > *Microbatrachus*
AHL, 1927	*Palmatorappia* > *Dendrobatorana*
NOBLE, 1931	Petropedetinae* > Cacosterninae* > Cornuferinae > Phrynopsinae
DECKERT, 1938	*Micrarthroleptis* > *Pseudarthroleptis*
LAURENT, 1940	*Coracodichus* > *Abroscaphus* > *Arthroleptulus*
LAURENT, 1941	*Taphriomantis* > *Elaphromantis* > *Heteropelis*
LAURENT, 1944	*Afrixalus** > *Heterixalus** > *Acanthixalus**
LAURENT & COMBAZ, 1950	*Cryptothylax* > *Phlyctimantis*
LAURENT, 1972	Kassinini > Leptopelini
DUBOIS, 1987*a*	*Ingerana* > *Kirtixalus* > *Taylorana* > *Amietia* > *Gorhixalus* > *Liurana* > *Bourretia*
DUBOIS, 1987*a*	Ranixalini > Ptychadenini > Tomopternini
PERRET, 1988	*Alexteroon* > *Arlequinus* > *Chlorolius*
CHANNING, 1989	Buergeriinae > Tachycneminae
FEI et al., 1991	*Odorrana* > *Glandirana* > *Rugosa* > *Pseudorana* > *Tenuirana* > *Unculuana* > *Quadrana* > *Tigrina*
DUBOIS, 1992	*Amo* > *Sylvirana* > *Nidirana* > *Afrana* > *Amnirana* > *Ombrana* > *Sierrana* > *Nasirana* > *Pulchrana* > *Amerana* > *Pantherana* > *Humerana* > *Papurana* > *Sanguirana* > *Blommersia* > *Brygoomantis* > *Guibemantis* > *Spinomantis* > *Quasipaa* > *Annandia* > *Eripaa* > *Gynandropaa* > *Feirana* > *Chalcorana* > *Aquarana* > *Zweifelia* > *Aurorana* > *Eburana* > *Tylerana*
DUBOIS, 1992	Limnonectini > Paini > Conrauini
BLOMMERS-SCHLÖSSER, 1993	Nyctibatrachinae > Indirainae
GLAW & VENCES, 1994	*Phylacomantis* > *Chonomantis* > *Pandanusicola* > *Ochthomantis*

several simultaneous names was based on the following rationale: in most cases, names currently in use were afforded priority over names currently considered invalid, and well-known names over poorly known names; priority was usually given to names designating groups of larger size (with more species) than others, and to names designating genera over names proposed for subgenera; junior homonyms and objective synonyms (e.g., unjustified emendations) of potentially valid names were given lowest priority; all other things being equal, I have preferred euphonious or elegant names to disgracious ones.

Of course, most of these first-reviser actions will have no bearing on future nomenclatures in these families, because synonymies between simultaneous names will be rather rare, even for the authors who currently advocate a very strong "lumper" approach to higher taxonomy. thus, to take just one example, it is highly unlikely that the names Buergeriinae Channing, 1989 and Tachycneminae Channing, 1989 will ever compete for synonymy! However, in other cases the problem will certainly arise, and it will be simpler and easier to refer to a single couple of tables to know the relative situation of two given names, rather than having to embark on long and difficult researches, so that these tables embrace all cases of "simultaneous" family-group and genus-group names in these families.

"*Rana duboisi*" in Emerson & Ward (1998)

Peer review by colleagues before acceptance of a paper for publication in a scientific periodical or journal is now a common practice worldwide. Manuscripts thus submitted for advice to scientists, who usually work in the same research field as the author of the paper, are sent to them under the (usually tacit) agreement that the reader will not make a private use of the information contained in the paper and will not publish this information, or information derived from it, prior to the publication of the submitted paper. With some shocking exceptions, this rule is usually followed by reviewers. But a particular problem may arise when the submitted paper is rejected, at least in its original form, and is never published, or only published after a considerable delay. The risk exists that the referee, either by inadvertency or by lack of request for information, might consider that, after a certain time has elapsed, the paper was actually published, and might feel free to use the information it contained, or to refer to this information as if it had been published. In some cases, this merely has the consequence of publishing only the final result of a work, without all the accompanying data that allow to ascertain that these results were obtained in a serious scientific manner; in some other cases, this may have nomenclatural consequences.

An example of the first kind is Kuramoto's (1990) mention of some of the results of a work by Iskandar et al. (unpublished) that he quoted as being in "*Alytes* (in press)", but that was actually never published or sent to press. The manuscript in question was indeed submitted to the journal *Alytes* on 4 April 1989, accessioned under number 89.156 and sent for review to two referees, including Mitsuru Kuramoto, on 12 April 1989. On 31 August 1989, after receipt of the reports of the two readers, copies of these reports were sent to the first author of the manuscript with an accompanying letter stating that, on the whole, the manuscript was very interesting and useful, but asking for a few minor modifications before

the paper could be published. Despite subsequent requests for the final manuscript, sent to the first author on 9 March 1990 and 18 October 1991, no second version of this paper was ever sent to *Alytes*. To the best of my knowledge, this paper was never published elsewhere. All the information contained in the original manuscript has therefore remained unpublished. Mention by KURAMOTO (1990) of some of this information is equivalent to mention of unpublished data obtained from a colleague through "personal communication", with the difference that in this case the communication was not direct between two colleagues, but went through the "mediation" of a journal editor. I suggest that colleagues who might wish to use these data should quote them as "ISKANDAR et al. in KURAMOTO (1990)".

In some cases, publication of previously unpublished taxonomic information may have nomenclatural consequences. This does not occur when no name is associated with the taxonomic information. Thus, EMERSON (1996: 279) wrote: "a new species has been discovered in Sulawesi in which the females retain fertilized eggs and the tadpoles develop in the body of the female (ISKANDAR, 1996)." This information seems quite interesting indeed, and readers may wish to know more about it. In the *References* of EMERSON's (1996) paper, the title of a paper by "ISKANDAR, 1996" appears, followed by the mention "*Alytes* (in press)". However, as of today (16 April 1999), no manuscript under this title (or an approaching one) has ever been submitted to *Alytes* for publication, so that this reference (ISKANDAR, unpublished *a*) could well be qualified as a "phantom reference" (see below). This may be quite frustrating for the reader but at least, from a nomenclatural point of view, there is no disturbing consequence, as the name of the "new species" was not mentioned.

The situation is different in the case of EMERSON & WARD's (1998) article on frogs of the "*Rana grunniens* species group". This paper starts (p. 538) with a table 1 presenting a list of species referred to this group. This table has a striking particularity: scientific names of species appear there "nude", i.e. without their authors and dates. As was well explained by NG (1994), citation of author and date is not only a tradition in zoology, it is important as it allows unambiguous identification of the nominal species at stake. Absence of such a basic information in a table published in a journal having "Linnean" in its title is an interesting illustration of a recent trend for zoological publications to neglect or fully ignore the basic rules, recommendations and needs of nomenclature. In this case, reference to DUBOIS's (1987*a*) work can allow the reader to avoid confusion, but without going to this reference it is impossible to know e.g. if the nominal species referred to in this table as "*Rana microtympanum*" is *Rana microtympanum* Van Kampen, 1907 (a member of *Limnonectes*) or *Rana microtympanum* Boulenger, 1919 (a member of *Hildebrandtia*). This table contains 14 of the 15 names listed by DUBOIS (1987*a*: 63) as members of his *Limnonectes* (*Limnonectes*) *grunniens* group, but the name *Rana macrodon* has disappeared from this list without explanation. Transfer of all these species from the genus *Limnonectes* to the genus *Rana* would have required a change of the grammatical gender of some of the specific names, which was not done in all cases, so that the list contains two incorrect spellings (*Rana modestus* for *Rana modesta*, *Rana visayanus* for *Rana visayana*). Later in the paper, p. 540, at the beginning of the *Material and methods*, two additional names suddenly appear for two species of this species-group: *Rana macrodon* and "*Rana duboisi*". This latter name is given without any explanation or reference to its source. It appears again on three occasions in the paper (p. 545, 546, 553), without further information. No publication proposing this name for a new species was ever published from 1758 to 1998, year of publication of EMERSON & WARD's (1998) work, so that

this name in this paper must be considered a new species name – unless it was borrowed from some unpublished manuscript, not cited in the *References* of their paper.

Actually, I am aware of two unpublished manuscripts where this name, or a related one, was proposed as the name of a new species: in the first one (DAS, unpublished), the name "*Rana duboisi*" was proposed for a new species of *Rana (Sylvirana)* (sensu DUBOIS, 1992) from above Kallar (Kerala, India); in the second one (ISKANDAR, unpublished *b*), a new species of *Limnonectes (Limnonectes)* (sensu DUBOIS, 1992) from Kamarora (Lore Lindu National Park, Central Sulawesi, Indonesia) was described as "*Limnonectes duboisi*". To the best of my knowledge, none of these two papers has been published so far, and it is not even certain that they were ever submitted for publication. At the head of the manuscript of ISKANDAR's paper (a copy of which was presented to me by Georges PASTEUR), it was stated that this paper was intended for submission to the journal *Alytes*, but as of today this has not yet been done. However, it is very likely that the name "*Rana duboisi*" was borrowed (and modified, through change of generic allocation) from this second manuscript, as the origin given for the specimens of this species studied by EMERSON & WARD (1998: 553) is the same (except for the misspelling "Linu" for Lindu) as that of the type-locality of "*Limnonectes duboisi*" in ISKANDAR (unpublished *b*), and as EMERSON & WARD (1998: 551) thank Djoko ISKANDAR for providing them with "tissue samples of Southeast Asian ranids". It would thus appear that EMERSON & WARD (1998), by publishing the name "*Rana duboisi*" before ISKANDAR, became the authors (in the technical sense of this term according to the *Code*) of this nominal species. However, this is not true, because this name is a nomen nudum in their paper: no character is provided to distinguish this species from related ones, nor is there any reference to a "bibliographic reference to such a published statement" (Article 13 a of the *Code*). This case is interesting, however, as it allows discussion of two questions that are likely to be raised again later in zoological nomenclature: is a species name rendered nomenclaturally available by publication, either (1) of a Genbank (or other similar data base) catalogue number reference for a sequence of this species, or (2) of a cladogram showing the hypothesized relationships of this new species to related taxa?

(1) EMERSON & WARD's (1998) paper does not contain any table or figure giving the sequences obtained for portions of the 12S and 16S ribosomal RNA genes of the specimens studied in their work. If it was the case, the sequences associated with the new name "*Rana duboisi*" would clearly qualify as diagnostic characters making this latter name nomenclaturally available, just like mating call characteristics (see e.g. SCHNEIDER & SINSCH, 1992; DUBOIS & OHLER, 1995: 179) or any other non-morphological character of an animal species. EMERSON & WARD (1998: 541) stated that the sequences obtained in their work were entered in the Genbank data base, and provided their catalogue numbers. As such a procedure is likely to become more and more common in evolutionary biology, it is important to know whether such Genbank cataloguing qualifies as a publication as defined by the *Code*. This is clearly not the case in the edition of the *Code* currently in force. Article 8 of this edition allows for a work containing a new name or a nomenclatural act to be regarded as published even if "produced after 1985 by a method that does not employ ink on paper in conventional printing", but only if it contains "a statement by the author that any new name or nomenclatural act within it is intended for permanent, public, scientific record". This does not apply to names entered in the Genbank, so that the latter must be considered as "unpublished" in the eyes of the *Code*. Caution will however have to be given to the precise wording of Article 8 in the final,

published version of the next edition of the *Code*, to check if this provision has not been changed. Sequences entered in the Genbank cannot therefore be used as diagnostic characters for new taxa, but it is important to note that, as soon as an author (either the person who established the sequence, or another colleague) publishes this sequence in a printed work, this sequence can become an excellent diagnostic character making a new name nomenclaturally available.

(2) Recent proposals have been made (see e.g DE QUEIROZ & GAUTHIER, 1994) to modify drastically the philosophical basis of zoological nomenclature by attaching the names to "phylogenetic definitions of taxon names", that would be more in agreement with a phylogenetic system of taxonomy than other kinds of "definitions" These proposals are in my opinion based on a major misunderstanding and entertain a confusion between *taxonomy* and *nomenclature*. In the system of zoological nomenclature currently in force, allocation of zoological names to taxa is not at all based on *definitions*, *diagnoses* or *descriptions*, but on the *taxonomic allocation of name-bearing type-specimens* or *onomatophores*, which constitute an objective, material and stable connection between the real world of animal populations and the world of language, whereas definitions are liable to change (for more details, see DUBOIS & OHLER, 1997) In this system, definitions or diagnoses only contribute to the *nomenclatural availability* of names, but not to their *allocation to taxa.* On the other hand, definitions or diagnoses are crucial for the qualification of taxa, and in this domain one may well wish to use "phylogenetic definitions", but this is a matter of taxonomy, not of nomenclature. Nomenclature is a system allowing a non-ambiguous, stable, automatic and universal allocation of names to taxa, under a given taxonomy, and the current rules of nomenclature are fully compatible with any taxonomic system, including the "phylogenetic taxonomy" (or "cladonomy" sensu DUBOIS, 1997) advocated by DE QUEIROZ & GAUTHIER (1990, 1992)

Under a nomenclatural system like that suggested by DE QUEIROZ & GAUTHIER (1994), the association of a name with a given clade in a cladogram, as is the case of the name "*Rana duboisi*" in figure 3 of EMERSON & WARD's (1998) paper, could possibly be considered enough to provide a "phylogenetic definition" of this name and to make it nomenclaturally available, but this is not true under the *Code* currently in force for all zoologists. Cladograms are hypotheses of *relationships* but, although built on the basis of a character analysis, they alone do not provide the *characters* of the included taxa. Under the *Code*, the presence of a diagnosis or definition, i.e. a statement regarding *characters* (not *relationships*), is necessary for a name to be nomenclaturally available, a taxon name published only with information on the supposed cladistic relationships of this taxon is therefore not available under the present *Code*.

In conclusion, the name "*Rana duboisi*" published by EMERSON & WARD (1998) associated with a reference to the Genbank and with a position in a cladogram, but without any diagnostic character, is, according to the current *Code*, a nomen nudum

To avoid the frequent repetition of similar nomenclatural puzzles in the future, it is highly desirable that authors try their best not to publish new names borrowed from unpublished manuscripts or from personal communications from colleagues (see e g. DUBOIS, 1998 20) Any name inadvertently published in such conditions may qualify, like in the present case, as a nomen nudum, i e. a name devoid of nomenclatural status, which has no real nomenclatural consequences. But it may also happen to be a validly published name, if it was associated in its

first publication with descriptive or diagnostic data, for which e.g. paper-printed gene sequences would fully qualify. Under the *Code* currently in force, such inadvertent publication of new names associated with gene sequences would give birth to nomenclaturally available, although unvoluntarily so, names, aptly qualified by VENCES et al. (1999) as "phantom names". This will remain so as long as the current *Code* is in force. Seemingly, as reported by VENCES et al. (1999), the next edition of the *Code* will include the following, highly desirable, new rules, that would greatly reduce the inadvertent creation of such names: (1) the need for an explicit statement that the new name applies to a newly defined species-group taxon; (2) the need for a clear designation of a name-bearing type, deposited in an identified collection. Such rules appear very reasonable and "obvious" for all experienced taxonomists, and, once in force, they would certainly be beneficial for the future of zoological nomenclature. But it is greatly to be hoped that these rules will only be prospective (i.e., applying to works published *after* the new edition of the *Code*) and will have no retroactive effect, otherwise, this might have dramatic consequences regarding nomenclatural universality and stability, as many names now considered valid by all zoologists were first published (either very long ago or more, sometimes much more, recently) without respect for these rules.

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